

# ANALYSIS

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I

THE PROBLEM OF CONTRARY-TO-FACT  
CONDITIONALS*By* JOHN WATLING

THERE is an argument which appears to show that it is impossible to verify a contrary-to-fact conditional; so giving rise to an important and puzzling philosophical problem. I shall try to solve this problem by showing in what way the argument is mistaken. There may be other philosophical reasons for holding that contrary-to-fact conditionals are meaningless, but with these I shall not be concerned. Now some writers suppose that the problem is about conditionals whose antecedent is false, some that it is about subjunctive conditionals, and some that it is about conditionals derived from scientific laws. Since the removal of this confusion is necessary to the solution of the problem, I want first to try to make clear the distinctions involved.

Suppose I assert a conditional whose antecedent concerns a day in the future, 'If it is windy to-morrow, then we shall go sailing'. The meaning of my statement does not depend in any way upon whether it is windy to-morrow: the only thing that depends upon the wind, if my statement is correct, is whether or not we go sailing. I make the same statement whether the antecedent is true or false. Let us call conditionals which have false antecedents 'contrary-to-fact': then the meaning of a conditional statement is independent of whether it is contrary-to-fact, just as the meaning of a categorical statement is independent of its truth. Whether or not this usage of the term 'contrary-to-fact', and of its equivalents 'counterfactual', and 'unfulfilled', is that which philosophers have adopted in the past, it is undoubtedly both natural and convenient.

Conditionals may be expressed in the indicative mood or in the subjunctive mood, but a difference of mood does not always involve a difference of meaning. 'If you go, then you will enjoy yourself' makes the same statement as 'If you were to go, then you would enjoy yourself' even though the latter suggests that probably you will not go. The distinction between expression in the subjunctive mood and expression in the indicative mood is one of grammar. The use of the subjunctive mood suggests that a conditional is counterfactual, where the corresponding indicative carries no such suggestion, but there is a distinction of meaning which the subjunctive mood some-

times, but not invariably, marks. Consider 'If Susie is listening at the door, then she is breathing very quietly' and 'If Susie were listening at the door, then she would be breathing very quietly'. The first could be asserted by someone who did not know how careful an eavesdropper Susie was, for it asserts no more than that either Susie is not listening or she is breathing quietly. It is the truth-functional conditional that is expressed symbolically by means of the horse-shoe sign. The second statement cannot always be decided by finding out whether Susie is listening at the door and whether she is breathing quietly. It cannot be decided by finding this out if she is not listening at the door. In such a case it seems impossible to verify the conditional, but we may collect evidence for or against it from a consideration of Susie's character. Does she always breathe quietly? Or, if she does not always breathe quietly, is she shrewd enough to do so when she is listening at doors? Some philosophers suppose that this evidence entails the statement that if Susie were listening at the door, then she would be breathing quietly, and argue that this is a statement of a causal connection or causal law: whether or not this is so, the statement is certainly not a truth-functional conditional.

I do not want to give a full discussion of what different kinds of conditional statement there are. In particular I do not want to discuss the distinction, amongst conditionals which are not truth functions, between those which assert causal connection and those which do not. There are great difficulties in the theory that all subjunctive conditionals assert causal laws,<sup>1</sup> and the theory is adopted only because of the difficulties about the verification of contrary-to-fact subjunctive conditionals which otherwise arise. If these difficulties are unreal, as I shall try to show, then there is no reason to suppose that all subjunctive conditionals assert causal laws, or causal connection. Of course very many *do* assert causal laws, and causal connection, and are intended to do so. In this article I shall assume that there can be contrary-to-fact subjunctive conditionals which are neither truth functions of their components, nor assert causal laws or causal connection. I make this assumption in order to show that the difficulties about verification, which are often supposed to follow from it, do not in fact follow.

However I do want to mention one other interpretation which some statements in the conditional form have been supposed to have—an interpretation in which they are not conditionals at all. I want to mention it because if all condi-

<sup>1</sup> See Nelson Goodman, *Fact, Fiction and Forecast*.

tionals were of this kind, then no problem about contrary-to-fact conditionals would arise. A statement of this kind shares neither the properties of a truth-functional conditional, which may be true merely because its consequent is true, nor those of a subjunctive conditional which may describe events which are possible, but not actual. This is an interpretation in which whether an assertion is made by someone using a conditional form of words, 'if p, then q', *does* depend upon whether p is true. Quine discusses it in *Methods of Logic*.<sup>1</sup>

"An affirmation of the form 'if p, then q', is commonly felt less as an affirmation of a conditional, than as a conditional affirmation of the consequent."

Anyone who makes a statement of the form 'if p, then q' is, in this usage, not stating that if p, then q; it is rather that if p, then he is stating that q. If the antecedent is true, then he asserts the consequent; if the antecedent is false, then he asserts nothing at all. No problems can arise over the meaning of the statement when the antecedent is false, for when what might be supposed to be the antecedent is false, then no statement is made. This same point can be put in a way that is clearer, but not so satisfactory: Anyone who says 'If I race, then I shall win' does not so much make an assertion as promise to make one. He promises that if he races, then he will assert that he will win. If he races, he is committed to the assertion that he will win; if he does not race, he is committed to nothing.

But it is evident that the majority of statements in conditional form are not of this kind, for, as Quine points out, we do not lose interest in them so soon as they have turned out to be contrary-to-fact. Indeed we often want to make inferences from a conditional that is contrary-to-fact, as a rather sly chess player might investigate his opponent's grasp of a position by asking 'And what would you have done had I not moved my knight?' I shall call conditionals which are neither truth functions nor conditional assertions 'subjunctive', whether or not they be expressed in the subjunctive mood; keeping the phrase 'grammatically subjunctive' for the purely grammatical property, to which, indeed, I shall have very little need to refer.

I have made three distinctions amongst conditionals, leaving aside the conditional assertion: one of fact, in accordance with fact—contrary-to-fact; one of grammar, indicative mood—subjunctive; and one of logic, truth-functional—subjunctive. Philosophical discussion should arise only over the third distinction, which is a distinction of meaning, but the problem

<sup>1</sup> Part I, Section 3.

has always been posed as about the meaning of contrary-to-fact conditionals and this suggests that a distinction of fact has been mistaken for a distinction of meaning. Now there is no problem about contrary-to-fact truth-functional conditionals, for these are true when their antecedent is false, and there is no problem about contrary-to-fact conditional assertions, for these make no statement when what appears to be their antecedent is false: the only problem is about contrary-to-fact subjunctive conditionals and it is this problem that I shall go on to discuss.

When philosophers assert that contrary-to-fact subjunctive conditionals are unverifiable they mean that it is *logically* impossible that observations should be, or should once have been, made whose record either entails that such a conditional is true or that it is false. They cannot mean merely that it is *in fact* impossible that such observations should be, or should once have been, made, for that it is *in fact* impossible to verify a statement by observation does not entail that that statement is meaningless. Various difficulties arise over the verification of contrary-to-fact conditionals; but most of these, such as the problems about the verification of statements about the past, or about the future, are not relevant here, since they are not peculiar to contrary-to-fact conditionals. The particular reason for which it is held to be logically impossible that a record of observations should entail either that a contrary-to-fact conditional is true or that it is false, is that it is held to be logically impossible that any record of events that actually have occurred, are occurring, or will occur, should either entail that such a conditional is true or that it is false; whether or not the record of actual events is obtained by observation. Now only if it is *logically* impossible that a record of events that actually occur should either entail the truth, or the falsehood, of a contrary-to-fact conditional, will it follow that it is logically impossible to verify such a conditional by observation. Therefore those who hold it logically impossible to verify a contrary-to-fact subjunctive conditional must hold, not merely that as a matter of fact no record of actual events either entails the truth, or the falsehood, of such a conditional, but rather that it is logically impossible that such a record should do so. For example, concerning the conditional 'If Mozart and Da Ponte had never met, then *Così fan Tutte* would not have been written' they must hold not merely that the fact that Mozart and Da Ponte did meet and *Così fan Tutte* was written is irrelevant to the truth of the conditional, as indeed it is; but also that a record of actual events could not possibly be relevant, and this is a

mistake. It is logically possible that Mozart and Da Ponte should not have met and, if this had been so, then a record of actual events would have been relevant to the truth of the conditional: such a record of actual events might have been that Mozart and Da Ponte did not meet and *Così fan Tutte* was not written.

Not all philosophers who find difficulties in understanding how contrary-to-fact subjunctive conditionals can be meaningful agree that their difficulties arise from problems about verification. Nelson Goodman<sup>1</sup> claims that counterfactual assertions and entities which are possible but not actual are unacceptable to him unless they can be explained and he asserts that he cannot justify this basic philosophical perplexity. If this is so, then what I have to say is irrelevant to Goodman's problem. I think that Goodman should try to say what gives rise to his perplexity, but if he does he may find that his reasons for believing that it is impossible to talk about things which do not exist have nothing to do with verification. But he sometimes suggests<sup>2</sup> that his problems do arise from problems about verification.

So far, then, I have argued that when philosophers hold that contrary-to-fact subjunctive conditionals are unverifiable they do so because they believe it logically impossible that a record of what events actually occur should either entail that such a conditional is true or that it is false. Now the whole point of my argument is that this is not a logical impossibility. In summary, the argument is as follows: The truth of a contrary-to-fact conditional is not determined by the truths of its components. The truth of a conditional that is not contrary-to-fact is determined by the truths of its components. But it is logically possible that a conditional that is contrary-to-fact should not be contrary-to-fact. Therefore it is logically possible that the truth of a contrary-to-fact conditional should be determined by those of its components. Therefore the fact that a contrary-to-fact conditional is not determined by the truths of its components provides no reason for the conclusion that it is logically impossible to make observations which would verify the conditional and no reason for the conclusion that such a conditional is meaningless.

I will now state the argument in greater detail: It is true that, for a contrary-to-fact subjunctive conditional, the answer to the question 'are the antecedent and the consequent true?' is not logically relevant to whether the conditional is true or

<sup>1</sup> *Fact, Fiction and Forecast*, p. 37.

<sup>2</sup> *Ibid.*, p. 14.



false; no record of what events actually occur either entails that the conditional is true or that it is false. But it is not logically impossible that a conditional which is contrary-to-fact should not be contrary-to-fact. (No conditional may be both, but the very same conditional may be either. That a conditional is contrary-to-fact entails that it is not in accordance with fact, but it does not entail that it is logically impossible that it should be in accordance with fact. That Caesar crossed the Rubicon entails that he did not leave the Rubicon uncrossed, but it does not entail that it was logically impossible for him not to have crossed it. A statement logically implies that its negation is false, but it does not logically imply that its negation is logically impossible. It is logically possible that a conditional which has a false antecedent should have a true antecedent, just as it is logically possible that a man with grey hair should have brown hair.) But, of a conditional which is not contrary-to-fact, a record of actual events *does* entail either the truth or the falsity: the answer to the question 'Are the antecedent and consequent true?' is logically relevant to the truth of a conditional that is not contrary-to-fact. Therefore it is not *logically* impossible that the truth of a conditional that is contrary-to-fact should be entailed by a record of what events actually occur. Although no conditional may be both contrary-to-fact and verified, yet it is logically possible that a contrary-to-fact conditional should be verified, for it is logically possible that a contrary-to-fact conditional should not be contrary-to-fact. That a conditional is contrary-to-fact entails that it is not determined by the truths of its components: it does not entail that it is logically impossible that it should be so determined. If a conditional is contrary-to-fact, then no observation of actual events will decide whether it is true or false; but it does not follow that it is not either true or false, for the decision, though impossible in practice, is not logically impossible: it is only prevented by the contingent fact that the antecedent is false, by the contingent fact that the conditional is contrary-to-fact.

Therefore this argument that contrary-to-fact conditionals are meaningless is mistaken. The mistake arises from failing to perceive that the meaning and the verification of a subjunctive conditional are the same whether it is contrary-to-fact or whether it is not. Consider 'If I catch the bus, then I shall be in time'. Before I set out for the bus it is obvious how to test the statement. After I have set out for the bus and missed it, it is still obvious how the statement might have been tested. A matter



of fact, not of logic, prevented the verification: the fact that I did not catch the bus.

The only conditionals whose verification is logically impossible are those for which it is logically impossible that their antecedent should be fulfilled: those, that is, whose antecedents are contradictory. For all other conditionals it is a matter of fact and not of logic which prevents their verification. A conditional makes the same statement whether its antecedent is true or false, but if its antecedent is false, then, because of this contingent fact, the conditional cannot be verified.

An example will make this clear. Recently I had new glasses prescribed for me. These were so strong that they made my eyes ache and I stopped wearing them, believing that they would harm my sight. In fact, even though I did not wear the new glasses my sight got worse. Now if I had worn the glasses and my sight had got worse I would certainly have asserted the conditional 'If I had not worn these glasses, then my sight would not have got worse'. This conditional would have been contrary-to-fact and neither it, nor its negation, would have been entailed by any description of what actually happened. But, since I did not wear the glasses, the antecedent of the conditional was fulfilled, and the fact that I did not wear the glasses and my sight got worse entails that it was false. Decision of the conditional by decision of its components would not have been logically impossible, in fact this decision took place.

The occurrence of events which falsify the antecedent of a subjunctive conditional makes it impossible to determine the truth value of the conditional from those of its components in much the same way as a high wall makes it impossible to see what is on the other side. The wall prevents one from finding out, by seeing the fruit, whether the fruit on the other side is ripe. Similarly lack of sun prevents one from finding out, by seeing the fruit, whether if there had been any sun, the fruit would have ripened. Subjunctive conditionals are about what would happen if something else happened, and what does happen may be logically relevant to this or it may not. When a subjunctive is unfulfilled there are very often other ways by which we can know whether or not it is true. It may even be that the truth values of the components are factually relevant to the truth of an unfulfilled subjunctive conditional. The operation of an ordinary electric light switch is an example of this. Suppose that I do not know, before entering a room, whether when the switch is up the light is on or off. Suppose

that, on entering, I find the switch down and the light on. Then, although the switch is down, I can assert 'If the switch were up, then the light would be off'. 'The switch is down and the light on' is not logically relevant to the truth of the contrary-to-fact conditional but it is factually relevant: if the switch is down and the light on, then if the switch is up the light is off.

Had philosophers noticed that a conditional whose antecedent refers to a future possibility is as likely to be contrary-to-fact as one whose antecedent refers to the past they would not have been so prone to confuse contrary-to-fact conditionals with subjunctive conditionals. It would be very odd to maintain that the statement 'Should Jack go skating on that pond, he will fall through the ice' is meaningful and would be verified by Jack's going skating and falling through, and yet be prey to such doubts about the verification and significance of the same proposition asserted at a later date, 'Had Jack gone skating on that pond, then he would have fallen through'. The fact that the problem has usually been stated for conditionals about the past, rather than for those about the future, can be explained. The difficulty for conditionals about the past arose from not perceiving that, though we cannot change the past and so turn a contrary-to-fact conditional into one which is in accordance with fact, yet the past might have been different, the antecedent of the conditional true instead of false, and the conditional verified. The conviction that we cannot change the past blinded philosophers to the fact that we might not need to do so. Since they did not suppose it impossible to change the future, they did not suppose it impossible to arrange for the fulfilment of conditionals about the future and so to verify them.

After this discussion it should be clear that the problem about the verification of contrary-to-fact conditionals has been removed. For whether a conditional is contrary-to-fact or not is independent of its meaning. The problem of the verification of subjunctive conditionals has been solved: for it has been shown that it is always logically possible that such a conditional should have been verified by events that took place. The problem was created by confusing the truth: 'It is logically impossible that a proposition should be both contrary-to-fact and verified by decision of its components' with the falsehood: 'It is logically impossible that a proposition that is contrary-to-fact should be verified by decision of its components.'

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## CAUSAL DISORDER AGAIN

By ANTONY FLEW

TWO further contributions to the controversy touched off by the Symposium on 'Can an Effect precede its Cause?'<sup>1</sup> have been published in *ANALYSIS* recently. I wish to make some comments on each of these.

## I

(a) In his 'Randomness and the Causal Order'<sup>2</sup> Mr. Michael Scriven presents as his example an experiment in which a group of subjects all draw by guesswork on a Monday 'copies' of a drawing which is only made and pinned up in the experimenter's study after its subject has been determined by a randomizing procedure on the following evening. Supposing "that there is a statistically very significant correlation between the drawings done on Monday and the drawing chosen on Tuesday"<sup>3</sup> he argues that it would be "very difficult to deny that the Monday guesses are partially determined by the Tuesday drawing: since they are (a) correlated with it, and (b) it is not correlated with any Monday (or earlier) physical situation which could serve as a common antecedent cause"<sup>4</sup>. Boldly he proceeds to claim "that I am not implicitly recommending a new sense of 'cause' here: I do not really think that a new sense of 'cause' is involved, although the term is being applied in circumstances that are very unlike the usual ones"<sup>5</sup>.

Now when the term 'cause', and its logical associates 'effect', 'affect', 'determine' (causally), and so forth, are used in their accepted senses it follows from 'A is a part cause of B': both that 'All other things being equal B will not occur without A'; and that 'Whenever A and the other supplementary circumstances required to complete this partial cause, then B'. Hence if we want to bring about B all we need to do is to bring about A and whatever supplementary circumstances are required to constitute with A a (causally) sufficient condition of B, and this combination will duly give rise to B. Therefore if "the Monday guesses are partially determined by the Tuesday drawing"<sup>6</sup> it would seem to follow that, supposing a significant

<sup>1</sup> *PAS* Supp. Vol. XXVIII: Dummett pp. 27-44; Flew pp. 45-62 (Flew I).

<sup>2</sup> *ANALYSIS*, Vol. 17, No. 1 (October 1957), pp. 5-9.

<sup>3</sup> Scriven, p. 5.

<sup>4</sup> p. 8.

p. 8.

<sup>5</sup> Scriven, p. 8.

proportion of the guess drawings on Monday were of tricycles and there was no significant concentration on any other subject, and supposing further that the experimenter while maintaining all the other experimental conditions proceeded to pin up on Tuesday a drawing of a Jaguar XK140, this would result in the Monday drawings containing the significant proportion of something other than tricycles, *viz.* Jaguar XK140's, which *ex hypothesi* they did not contain.

Scriven's claim to be using 'cause' in the ordinary sense may be bold, but he is not so rash as to run directly into such a blank contradiction as this. After making it he goes straight on to admit: "Certainly it would be awkward [*sic*] if we were obliged . . . to say that we can now alter the past; we should rather say that the past *was determined* (partially) by what we now do, a claim which will certainly be invalidated if we act in a way which is usually correlated with past events of a kind other than those we believe to have occurred".<sup>1</sup> To avoid collapse of this sort Scriven has prudently forearmed himself with a prophylactic; by stipulating that the subject of the target drawing in his experiment must be determined by a random selector. Thus to "the most striking objection . . . [which] arises when one considers the possibility that one could select the Tuesday drawing so that it gave no significant correlation with the Monday guesses" he is able to reply "that to do this is to abandon the experimental design in terms of which the reversed order is being explicated"<sup>2</sup>: since if the experimenter determines the subject of the picture in the light of the distribution of subjects among the guess drawings that subject is not being determined by a random selector. So after considering the possible results of such a further *hitting*<sup>3</sup> experiment he tells us: "I do not regard these considerations as relevant to the case where the Tuesday drawing is randomly determined yet correctly guessed".<sup>4</sup>

Scriven hopes thus to dispose in advance of all objections on these lines by the provident stipulation that his candidates for the description "reversed cause" must be themselves randomly determined. So whereas all other causes are levers by which their effects may, at least in principle, be brought about or prevented, reversed causes could not, even in principle, be used either to bring about or to prevent their effects. Yet once it is fully understood that Scriven's cunning stipulation

<sup>1</sup> Scriven, p. 8: italics his.

<sup>2</sup> Scriven, p. 7.

<sup>3</sup> Flew I, p. 57.

<sup>4</sup> Scriven, p. 7.

must sterilize necessarily all the usual implications of efficacy, his suggested description of his example in terms of causation reversed is seen as at worst contradictory and at best pointless. By choosing to develop it from an actual case rather than to excogitate something entirely imaginary in complete philosophical isolation he escapes one general implied criticism of Dummett,<sup>1</sup> which fairly he extends to Black.<sup>2</sup> By choosing to start from the particular case of Whately Carington's experiments with target drawings<sup>3</sup> he gains three particular advantages, which he points out.<sup>4</sup> But what he cannot escape is the dilemma presented to anyone who wants to speak of causes, or 'quasi-causes', occurring after their effects, or 'quasi-effects'. All such attempts, as I argued against Dummett at the beginning, "must: either be frustrated by contradictions; or end in a fraud".<sup>5</sup>

(b) Nevertheless I think that Mr. David Pears fails to do justice to Scriven's position. In one of the two footnotes which he directs at this he writes: "according to Scriven backward effectiveness does not enable a person to do something now in order to make something else have happened. But it is hard to see what is left of the concept of backward effectiveness when this feature has been subtracted from it."<sup>6</sup> But Scriven, significantly though perhaps not deliberately, said nothing about backward effectiveness, or efficacy, about reverse-order causes producing or bringing about their effects: he wrote only of "reversed cause-effect relationships",<sup>7</sup> "backwards determination",<sup>8</sup> "reversal of the causal order",<sup>9</sup> "the reversed order",<sup>10</sup> and "Monday guesses . . . partially determined by the Tuesday drawing".<sup>11</sup> What he actually maintained was this. Not that later causes might be causally *effective*, might *bring about* their effects; even though it had to be made by stipulation logically impossible to exploit this effectiveness to bring about a clustering of guesses around a particular value, by arranging for the target drawing to have that particular value. But that, although this was so, nevertheless it was proper, and indeed scarcely avoidable, to describe his imaginary experiment in terms of "reversed causation".<sup>12</sup> Scriven's mistake is, therefore: not, as Pears suggests, to insist on talking emptily of effectiveness and so

<sup>1</sup> Flew I, *passim*.      <sup>2</sup> Scriven, p. 8.

<sup>3</sup> 'Experiments in the Paranormal Cognition of Drawings' *Proc. SPR.* Vols. XLVI and XLVII.

<sup>4</sup> Scriven, pp. 8-9.      <sup>5</sup> Flew I, p. 48.

<sup>6</sup> 'The Priority of Causes' in *ANALYSIS*, Vol. 17, No. 3 (January 1957), p. 59, n. 1.

<sup>7</sup> Scriven, p. 5.

<sup>8</sup> p. 5.

<sup>9</sup> p. 6.

<sup>10</sup> p. 7.

<sup>11</sup> p. 8.

<sup>12</sup> p. 8.

forth where he has ruled out all possibility of giving to these terms their usual empirical meaning; but rather to insist on talking misleadingly of causation where he has subtracted an essential part, the idea of effectiveness, from the meaning of "cause".

These distinctions are rather more important than perhaps they seem. For while to subtract from the notion of effectiveness the idea of the possibility of bringing about or preventing would leave us with nothing but an animistic projection, subtracting the notion of effectiveness from that of causation does leave something of importance; although this remainder does not itself constitute causation. What remains are the ideas, first of constant brute-fact conjunction, and second, and containing the first, of lawful connection. The former can of course be analysed on Humean lines wholly in terms of material implication; not-as-a-matter-of-fact-A-and-not-B. The latter goes beyond this, and involves saying also that, if A had not been, B would not have been. Now from these two elements, the residue after we have eliminated from the notion of causation the idea of effectiveness, we can construct a new concept of "non-causal connection", which, as I have argued elsewhere, might conceivably find application in the handling of the 'precognitive' phenomena of parapsychology. "This would involve allowing that there might be laws of the form, 'In such and such conditions subjects of such and such a sort always (or to such and such a degree above bogey) guess right': *where the possibility of causal explanation . . . is ruled out*".<sup>1</sup> Although Scriven has failed in his intention to elaborate a case in which we might properly speak of reversed causation, he has, unwittingly, succeeded in providing one to which this concept of non-causal connection might appropriately be applied.

## II

Pears is concerned to trounce, with a fine thoroughness, all the five earlier contributions for various mistakes which he believes he detects in them. I will deal here only with points which he makes against me specifically, although certain of his other contentions seem very questionable.<sup>2</sup>

<sup>1</sup> 'Broad and Paranormal Precognition' III (v), to appear in the forthcoming *Philosophy of C. D. Broad* (Flew III. To keep in step with Pears I leave "Flew II" to cover 'Effects before their Causes—Addenda and Corrigenda' in ANALYSIS, Vol. 16, No. 5 (April 1956), though Flew III was in fact written considerably earlier.)

<sup>2</sup> Thus he says of our present concept of cause and its associates: "It is of the essence of this system that it should cover the whole world" (p. 62). But compare G. J. Warnock, 'Every Event has a Cause' in *Logic and Language* II, especially § IV.



(a) Pears writes: "Flew seems to think that his [Dummett's] thesis depends in some way on his inadequate analysis of causal connection. . . . I cannot see any force in this criticism."<sup>1</sup> Now Dummett had written: "On the ordinary Humean view of cause, a cause is simply a sufficient condition . . . [and] however we elaborate on the notions of sufficient and necessary conditions, the relation can hold as well between a later event and an earlier as between an earlier and a later."<sup>2</sup> I observed that from this Dummett drew "the conclusion: not that this 'ordinary Humean view' needs to be revised and supplemented; but that he might invent a new notion of (quasi-)cause, by reversing the time direction of the present one."<sup>3</sup> And then I proceeded to indicate the sort of thing which such an "ordinary Humean view" leaves out: namely everything which in the previous section of this paper I was subsuming under the ideas of lawful connection and causal effectiveness. The point of doing all this was to show that: whereas on a Humean analysis the provision that causes must precede their effects must, as Dummett saw clearly, appear to be arbitrary and alterable; when account is taken, as it should be, of the other two main elements in the concept it can be seen as not arbitrary but necessary. For effectiveness can only be forward: since the suggestion that something might bring about, might in principle be used to bring about, something else which has already either happened or not happened involves a hopeless contradiction.<sup>4</sup> So it reveals a complete misunderstanding to say that Dummett adopts this Humean analysis only "for the sake of simplicity, and its inadequacy has nothing to do with the direction of effectiveness."<sup>5</sup>

Pears in support argues that: "It cannot be a 'weak' analysis [of causal connection] . . . which leads Dummett to favour, in certain circumstances, the second of these alternatives [*viz.* backward effectiveness], since both alternatives [i.e. forward effectiveness and backward effectiveness] are equally compatible with the same analysis. . . ."<sup>6</sup> Now my contention was in any case: not that his acceptance of a Humean analysis alone determined Dummett's predilection; but that it was its defectiveness which made it seem possible to reverse the time direction ("the conclusion . . . that he *might* invent a new notion . . . by reversing the time direction . . .": not italics in original). While, as we

<sup>1</sup> Pears, p. 55.

<sup>2</sup> Dummett, pp. 27-28, quoted Flew I, p. 49.

<sup>3</sup> Cf. § I above; and Flew I, § C pp. 57 ff.

<sup>4</sup> Pears, p. 55.

<sup>5</sup> p. 55.

<sup>6</sup> Flew I, p. 49.



have seen, an analysis of causal connection which takes due account of the element of effectiveness will thereby entail that the time direction cannot be backwards, and will therefore be incompatible with "the second of these alternatives" [*viz.* backwards effectiveness]. (b) Pears writes: "Dummett would advance the hypothesis that L [a later event] is the quasi-cause of E [an earlier event] only if E appeared to have no cause and L's cause in no way depended on E. . . . But, Flew objects, the second of these conditions would be violated if the investigator tried to bring about or suppress L because he already knew about E; and he seems to think that this difficulty is insuperable. But in fact it would be easy for the investigator to keep himself in ignorance about E until he had finished with L."<sup>1</sup> Now Pears' criticisms have shown me that my remarks about "a peculiar elusiveness to experimental investigation" of "quasi-causal correlations"<sup>2</sup> were muddled. But this particular objection quoted will not do. For the whole point of the bilking experiment must be to suppress, or to arrange for a non-corresponding value of, L in the light of prior knowledge that E has occurred, and what its value was. If the experimenter keeps himself in ignorance of E till he has finished with L he cannot be bringing about or suppressing L *because* he already knows about E.

The position seems to be: not that Dummett's stipulations preclude all experiments; but that they rule out the bilking experiment. The suggestion of such an experiment is offered as a dilemma to the spokesman of the concept of quasi-causation. For either the experimenter could succeed, or he could not: if he could not, then his failure would be a reason for saying that E must after all be causally related to L; if he could, then his success breaks the correlation between E and L. Either outcome conflicts with one of the stipulations. However, this dilemma, precisely for this reason, can, at a price, be eluded. It is possible to insist on the stipulation that L must be causally independent of E (Dummett's Conditions Two and Three); perhaps adding to ensure this Scriven's proviso that L must be randomly determined. You can then say that such a bilking experiment, at any rate if it is successful, is necessarily irrelevant to the case you have in mind. The price of this manoeuvre we have already seen.

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<sup>1</sup> Pears, p. 56.

<sup>2</sup> Flew I, p. 57.

## 'WHAT-FOR' QUESTIONS AND THE USE OF SENTENCES

By CHARLES E. CATON

MR. ALAN R. WHITE, in a recent article,<sup>1</sup> argues that Professor Ryle is unjustified in holding<sup>2</sup> that one cannot ask whether a person knows how to use a certain sentence, because (among other things) Ryle's view arises from another mistake, "namely the denial that the purpose or function of an expression is part of its use".<sup>3</sup> I wish to argue that the issue between Ryle and White is unclear because they do not notice that there are at least two sorts of "what-for" question and that White's argument against Ryle, insofar as it depends on the point above, does not hold.

White agrees with Ryle that there is a distinction between "how" questions, questions about "the way, method or manner" in which an expression is used, and "what-for" questions, questions about "the purpose for which an expression is used" or "about what it is used for".<sup>4</sup> The latter half of this distinction, however, actually covers two quite distinct sorts of question. This can be seen most clearly by considering 'what-for' questions which are not about linguistic expressions. Both Ryle and White discuss such questions and seem to assume that their meaning does not differ from the same sort of question asked about linguistic expressions.

The two sorts of 'what-for' question are these: the first sort, whether the verb 'to use' is in the active or passive, does not mention a definite individual or group, while the second sort does. Examples of the first sort are 'what are hammers used for?' and 'what do people use hammers for?'. Examples of the second sort are 'what was the hammer used for by the defendant?' and 'what did the defendant use the hammer for?'. The two sorts of question may be represented schematically by 'what are X's used for?' and 'what did Y use that X for?', respectively.

Some important differences in meaning between these two sorts are these: (i) In the case of the first sort of 'what-for' question, if one is familiar with X's, the correct answer is obvious; in the case of the second, even if one is familiar

<sup>1</sup> "The Use of Sentences," *ANALYSIS*, Vol. 17 (1956), pp. 1-4.

<sup>2</sup> In his "Ordinary Language", *The Philosophical Review*, LXII (1953), pp. 167-186.

<sup>3</sup> White, *loc. cit.*, p. 1. <sup>4</sup> *Ibid.*, p. 3. Cf. Ryle, *loc. cit.*, pp. 173-184.

with X's the answer is not usually obvious. For example, to one familiar with hammers, the answer to 'what are hammers used for?' is obviously 'to drive nails'. But one's familiarity with hammers does not help one to decide what the defendant used his hammer for on the night in question, except insofar as it limits the number of purposes for which it is at all plausible to attempt to use hammers. (ii) In the first case, the question is about X's, not about the people who use them; that is, the question is about the purpose of X's. In the second case, the question is about Y, i.e. about his purpose. As a result, (iii) in the first case, the question is not specific about who uses X's, or when, or which X is used; while in the second case, the question *is* specific about these things. Also (iv) in the first case, for any given X, the correct answer usually mentions one or a small number of activities, i.e. those which X's are normally used to conduct or for which X's were made. In a question of the second sort, for any given X and Y, the correct answer might be any of the various activities in which a person can deliberately engage.

The result of these four differences is that some answers to 'what-for' questions would indicate that one of the sorts was being mistaken for the other or wasn't being understood fully. For example, to the question 'what are hammers for?' it would be not merely false, but a misunderstanding of the question to answer, 'well, to break windows, murder people, wreck machines, . . .' And to 'what was the defendant using the hammer for?' it would be wrong to answer 'to drive nails' merely because this is what hammers are for.

Let us turn to 'what-for' questions about linguistic expressions. If there are such questions (as it seems there are) and if they ask the same sort of question (as Ryle and White seem to assume), then there should be analogues, at least, of the four characteristics noted in the non-linguistic case. And there do seem to be these analogues. Thus in the case of type-one questions there is a number of activities which are normally conducted by using linguistic expressions, though in most cases the expressions were probably not "constructed" or invented for use in these activities. Examples of these are describing, referring, expressing our feelings, asserting, questioning, promising, swearing oaths, etc.<sup>1</sup> In some cases there is a difference in the obviousness of the answer, however. Although it

<sup>1</sup> Perhaps these wouldn't all be called "activities" normally, but I need a blanket term. They are all things which we use expressions for and this is what is here meant by calling them "activities".

is obvious to anyone familiar with hammers that they are for driving nails, it is not in the same sense obvious to everyone familiar with the expression 'that's true' that it is used (among other things) to agree with what someone has just said. However, it is, perhaps, obvious in the sense that they would recognize, when told, that this was one of its functions. It is clear that such questions are about what the expression is for or about its function or even, perhaps, about one of its purposes and not about the purposes people might have in using it or the reasons why they used it on some occasion. Such questions are not specific about the user, the occasion of the use, or the particular utterance of the expression. If these are mentioned at all, it is because they are a certain *kind* of person and context. And the expression being asked about is an "expression" in the sense in which the same expression can occur in many different utterances.

It seems, then, that 'what-for' questions of the first type, when asked about linguistic expressions, are closely similar to the same type of question asked about non-linguistic things. The same is true of the second type. The questions and answers of this sort are even more similar, in respect of the features mentioned, if not the same as those about non-linguistic things.

Now I think it can be seen that failure to notice the distinction between the two sorts of 'what-for' question introduces considerable ambiguity into both Ryle's and White's discussions. Ryle, in his discussion of use and utility,<sup>1</sup> suggests that a person may learn what a spark-plug is for (though he can't learn how to use it), that the answer to 'what is whistling for?' is 'nothing', and that the answer to 'what is a centime or peseta for?' is obvious, viz. that they are for making purchases. In all these cases he seems to have the first sort of question in mind. Take whistling: one can have many purposes in (or reasons for) whistling—to keep oneself company in the dark, to call one's dog, to attract someone's attention, to show someone what tune one is talking about, to divert oneself, etc. Any of these might, on occasion, be the answer to 'what were you whistling for just then?' or to 'what was he whistling for?' i.e. to a type-two question. It is sometimes necessary to ask this sort of question and often in no way obvious what the answer is, but Ryle says of the sort of 'what-for' question that he has in mind that it "can be asked, but it is seldom necessary to ask it, since the answer is usually obvious."<sup>2</sup> Ryle seems, then, to be making a point about the first type of 'what-for' question.

<sup>1</sup> *Loc. cit.*, pp. 173-174.

<sup>2</sup> *Ibid.*, p. 174.

White's examples seem to come from both sorts of 'what-for' question, but mostly from the second. Thus he accuses Ryle of using a mistaken analogy in taking pies as an example: "Cooks do use pies, for example, to feed their children, to impress their neighbours, to carry off prizes at the local show."<sup>1</sup> The occurrence of "for example" here suggests that White is giving examples of purposes that people might on particular occasions serve by using pies (just as I have above in the case of whistling). 'To impress her neighbours' and 'to win a prize at the local show' might well be answers to type-two 'what-for' questions. They are not necessarily, or even probably, obvious answers. They are certainly wrong answers to type-one questions about pies. But there is a certain obviousness about using a pie to feed one's children. 'What are pies (used) for?' is, I imagine, a question rarely asked, except by children or people from strange cultures; for the answer is obviously 'they're to eat' or 'they're food' (what food is for being even more obvious). Pies are normally used as food, this is what they were invented for (if they were invented), and everyone familiar with them knows this. The question is one of the first type, then.

It is more difficult to decide which type is in question when we come to White's examples involving linguistic expressions. He says: "We all do use sentences, for example, to describe scenes, to give accounts of journeys, to publicise the result of our research, to express our feelings, to issue orders, pass verdicts, etc."<sup>2</sup> Because these are offered as examples and followed by "etc.", we might be tempted to regard them as possible answers to 'what-for' questions of the second type. It is true that on many occasions people wish to do the things White mentions and they certainly use sentences (and *a fortiori* other sorts of expression) to do these things. There seems, however, to be some conflation of the two types here. 'To describe (things)', 'to express our feelings', 'to issue orders', and 'to pass verdicts' might be answers to type-one questions. But 'to describe a scene' sounds like the answer to a type-two question (abstracted from particular cases for the purpose at hand). The difficulty here is that without sample utterances we cannot tell whether the list is offered as a summary of particular purposes which people might serve by using sentences or as a list of linguistic activities in which sentences (of various types) are regularly used.

<sup>1</sup> White, *loc. cit.*, p. 2.

<sup>2</sup> *Ibid.*

Now I think it can be seen that, despite the ambiguities in the discussions of both Ryle and White, part of White's case against Ryle is not, as it stands, proved. For Ryle contends that one cannot ask whether a person knows how to use a certain sentence. And White argues that this view arises from the mistaken doctrine that the purpose or function of an expression is not part of its use. Now the doctrine said to be mistaken here may be either (i) that the purpose a person has in using an expression on a particular occasion is not part of its use, or (ii) that the purpose for which the expression is used (the function of the expression) is not part of its use. These doctrines are about answers to type-two and to type-one 'what-for' questions, respectively. I now wish to argue that doctrine (i) is true and that doctrine (ii) is false.

That the answers to type-two 'what-for' questions about linguistic expressions do not (purport to) state part of their use can be seen from a consideration of what these questions are about. We have seen that in the case of this sort of question, an answer may mention any of an indefinitely large number of purposes,<sup>1</sup> that there is no need for the things for which the expression is used to be things which are regularly done by using the expression, and that the users, the occasion of use, and the particular utterance are specified. Now questions about "the use" (without qualification) of an expression are not like this; the questions which are like this are questions about so-and-so's use of a particular expression on a certain occasion. Questions about the use of an expression are instead like type-one questions. They are about things that are regularly done by using the expression and they are not specific in the ways mentioned. Thus the same kind of question about an expression E can be raised by asking 'what is E used for?', 'what is the function of E?', 'how is E used?', 'how do we use E?', or 'what are the uses of E?'. (This shows, incidentally, that the distinction on which White says he agrees with Ryle should be between 'how' questions and type-two 'what-for' questions, type-one 'what-for' questions being one variety of 'how' question.) One cannot raise this kind of question by asking 'what did Jones use E for yesterday when he was talking to Smith?', 'why did Jones use E . . .?', or 'what was the point (purpose) of Jones's using E . . .?'.<sup>2</sup> These are type-two questions, while the first set are type-one questions.

<sup>1</sup> This fact makes White's phrase "the purposive use" (*loc. cit.*, p. 4) misleading.

<sup>2</sup> 'How was Jones using E?' or 'what did Jones mean by E?' raises a different type of question, one similar to 'how do we use E?' but only about Jones.



It follows that White's argument against Ryle, insofar as it consists in contending that it is a mistake to deny that the purpose of an expression is part of its use, fails to hold. For, though Ryle's examples suggest that he is talking about type-one questions, he *may* wish to deny only that the answer to type-two 'what-for' questions state part of the use of a sentence, and this is true, for they do not state part of the use of any expression. That is, they are not about the sort of thing referred to by 'the use' (without qualification) of an expression. Or, if we insist on saying they do state part of "the use" of sentences, then this sort of "use" is so different from the other sort that the similarity is only in name.<sup>1</sup> And even if we decide to talk about "the use" of sentences, we shouldn't, I think, allow the phrase to blind us to a clear distinction between two sorts of question.

The issue remaining between Ryle and White now appears to be whether or not the answer to a type-one 'what-for' question about a sentence states part of its use. It is not clear that White has asserted this, since he fails to distinguish the two sorts of question and his examples do not enable us to decide. But it does seem that Ryle would not assert it, since he denies that there is such a thing as the use of a sentence.<sup>2</sup>

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<sup>1</sup> I am not denying either that the verb 'to use' or that the noun 'use' has uses in connection with sentences—even everyday uses (e.g., 'But the sentence you actually used was "..."). My point is about the philosophical use of the phrase 'the use of the sentence "...".

<sup>2</sup> That is, he denies that we can ask whether someone knows how to use a certain sentence (*loc. cit.*, p. 178).



## A NOTE ON DECEIVING

By BETTY POWELL

I WISH to comment on Professor Austin's treatment, in his broadcast talk on "Performative Utterances",<sup>1</sup> of promising when one does not intend to do what one promises to do, saying 'I do' before a clergyman when one is already married, and so on. I shall consider his treatment in the light of his remark that there are certain verbal forms which we do not have. The example he gave was 'I insult you', and he suggested that we do not have this form because we disapprove of insulting and so have not evolved a form 'I insult you'.

This may be so, but there are more illuminating forms which we do not have; for example, 'I lie to you', 'I deceive you', 'I hint', 'I insinuate', etc., and the absence of these is not simply to be explained by the fact that society disapproves of deceiving, lying, etc. These activities, unlike promising, cannot be referred to both in the present and in the past tense. In the case of promising, once I have said 'I promise', whether or not I do what I promise to do, and even if I have never had any intention of doing it, at some future date I refer to this by saying 'I promised'. Now I cannot say both 'I deceive you' and 'I deceived you'; 'I lie' and 'I lied to you'; 'I hint' and 'I hinted to you'. The use of the past tense implies that I never used the present, although if I have deceived you, I must at some point have been deceiving you. We do not use the present tense in these cases, since, if we did, we could not perform the activities of lying, deceiving and hinting.

In order to lie to you, I must tell you something which I know or believe not to be true, as if it were true. I do not give the game away by saying 'I am lying when I tell you that...', although I may in fact be lying when I tell you that. . . . I may say 'I hinted that he should raise my salary' or 'I was hinting that you should raise my salary', but I do not say 'I am hinting that you should raise my salary'. Whatever I should then be doing, I should not be hinting.

It would indeed be odd to say 'I promise to come but I don't intend to come'. To promise to come without intending to do so, is to do more than promise; it is to make a false promise, or, to avoid misunderstanding, for I do not wish to

<sup>1</sup> August 24, 1956. I am indebted to Professor Austin for allowing me to read the transcript of his talk.

suggest that we pretend to promise, it is to promise in order to deceive. What one is also doing is deceiving and not simply promising, and one could not deceive if one said 'I promise to come but I don't intend to'.

In making false promises one uses the linguistic conventions which govern the use of the phrase 'I promise', just as one uses the conventions which govern the use of the word 'know' if one lies about what one knows. One relies on the fact that if one says 'I know' other people will take one's word, or if one says 'I promise to be there' other people are entitled to expect that one will be there. One does one particular thing—one promises—in order to do another—to deceive.

If, in the appropriate place, i.e. in a witness box, one swears to tell the truth, the whole truth and nothing but the truth, the judge and jury are entitled to expect that what one subsequently says will be the truth. If one swears to tell the truth with the intention of lying, one swears falsely; one swears to tell the truth in order to deceive the judge and jury, and of course, one commits perjury.

If a man utters the words 'I do' in the appropriate place (before a clergyman or registrar) at the appropriate point in the wedding ceremony, he implies, amongst other things, that he is not married or within the prohibited degrees of relationship—or better still, these particular words uttered in a particular place at a particular time, entitle the hearers to infer that the speaker is not married or within the prohibited degrees of relationship. If he is married, he deceives a number of people by using a particular formula or engaging in a piece of ritual by which he gives them to understand that he is not married etc. And of course, he does more than deceive; he commits bigamy.

It is precisely because certain words uttered in the appropriate place do arouse certain expectations that one is able to deceive. How else could one commit bigamy except by uttering the words 'I do' in the appropriate place and thus giving people to understand that one was not married? How else could one pervert the course of justice except by swearing to tell the truth and then lying? One could not mislead the court if one announced one's intention of doing so. If a man wants to commit perjury, then the correct formula to use is 'I swear to tell the truth etc.', just as, if he wants to commit bigamy, the correct formula to use is 'I do' in the appropriate place.

There are therefore no formulae—ritual phrases—for such activities as deceiving, lying etc., and no expressions which are reducible to 'I am deceiving you', 'I am lying to you', 'I am committing bigamy' etc. Yet deceiving and lying are things that we do for which a public utterance is required. One cannot lie by saying something under one's breath. We must use certain formulae like 'I promise', 'I know', 'I swear'.

The question is, is the best way of describing activities of this sort to say, as Austin does, that they are "infelicities"? It is questionable whether the most useful thing to say about a man's uttering the words 'I do' before a clergyman when he is already married is that he performs an act which "misfires"—fails to come off. One could be a successful bigamist. And of course, acts of deception may themselves fail to come off. To take this line is to obscure certain important differences. Misinforming, for example, is not always failure to inform; it is sometimes successful misinforming.

To go beyond saying that in performing certain activities one *uses* certain accepted procedures and to say that one *abuses* the accepted procedures is surely to introduce a moral point of view into an analysis of language and the activities we perform in using the language. To say that certain verbal procedures e.g. 'I promise' are intended for use by people who have certain intentions and that if one uses these formulae without having the intentions one abuses the procedure, is surely to make a moral pronouncement. If one wants to deceive there is no alternative linguistic procedure one could adopt.

Deceiving, lying etc., are things that we do, and for which a public utterance (deceiving sometimes, lying always) is required. As such they need to be examined in their own right, and not merely considered as "misfires" or "abuses", or indeed any sort of "infelicity".

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